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Parthenogenesis in the Honey Bee.

BY PROF. C. T. E. VON SIEBOLD.

[CONCLUSION.]

Notwithstanding the experiments hitherto made in a practical way by which Dzierzon's theory has acquired the right of asserting its justice, we cannot reject the demand that, by means of direct experiments, we must acquire the conviction that the drone eggs require no fertilization for their development, whilst the same eggs, in order to furnish female or worker bees, must be fecundated; for it is only by such strict scientific proofs that this new theory will acquire a firm and secure basis.

Since the production of fishes by the artificial fecundation of the eggs had been carried on of late years with such fortunate results, it was natural to think whether it was not possible to establish the correctness of Dzierzon's theory incontrovertibly by the artificial impregnation of the bees' eggs. But this mode of proof was necessarily given up again at once as impracticable, for whoever submits the eggs of bees to a close examination will immediately see that these eggs, from their extreme delicacy, are quite unfitted for such experiments. There would be no possibility of extracting mature eggs uninjured from the ovaries, in order to transfer them either unfecundated or artificially impregnated into cells to be taken care of by the bees. Nor would these delicate eggs bear without injury the contact of a brush, however fine, moistened with the semen of male bees, as would be required for artificial impregnation. Prof. Leuckart proposed to employ eggs which had already been laid as drone eggs in drone cells, and to fertilize them artificially afterwards, in order in this way to decide the question, whether we should succeed by artificial impregnation in developing such eggs into workers or queens. He called attention, however, at the same time, to the difficulties which are opposed to the success of this experiment. He justly pointed out that only very fresh and newly deposited drone-eggs should be made use of for artificial fecundation, for as soon as the thin albuminous coating, with which the eggs of in-

sects are laid, becomes dry, which certainly takes place quickly on deposited eggs, the semen employed for artificial impregnation can no longer penetrate through the pores of the egg shell into the interior of the egg, by which means alone as will be shown hereafter, the fertilization of the eggs of insects can be completed. From the importance of the object which would be attained by these experiments, difficult as they are to carry out, I heartily join in Leuckart's wish that such experiments should be undertaken by many hands; perhaps one or the other of the experimenters would be so fortunate by the concurrence of several favorable accidents as to attain what from Dzierzon's theory must *a priori* be expected as the result. From the preceding statements it follows that the artificial impregnation of bees' eggs could not as yet be employed in favor of Dzierzon's theory.

Very different hopes were awakened in this respect when we became acquainted with the existence and office of the micropyle of the eggs of insects. Since Leuckart and Meissner have seen the spermatozoids penetrate the egg-shells through their peculiar openings into the interior of the eggs of insects, we must say beforehand that if Dzierzon's theory proves to be correct, this process can only be observed in those eggs of bees which are destined for evolution of females or workers; and that in the eggs which remain unfecundated, from which only drones are developed, no spermatozoids will penetrate through the micropyle, whilst the micropylar apparatus must exist in exactly the same degree of development in all these eggs, as all eggs are originally of one and the same kind and nature.

Those eggs of bees which have to undergo a fertilization are fecundated at the moment when they slip past the orifice of the seminal duct of the receptacle within the oviduct, (vagina). At this moment, as we may certainly suppose, some spermatozoa are pressed forth out of the efferent duct of the seminal receptacle, and these in this way by means of their mobility find an opportunity of penetrating through the micropylar apparatus into the interior of the egg. That the act of fecundation of the insect egg actually takes place at the point of the vagina just mentioned, was asserted by me in the year 1837, and confirmed by an observation which I made upon

Musca vomitoria. In *Musca vomitoria* and its allies, which had not yet completed the business of oviposition, or perhaps had been disturbed in it and had not immediately met with another suitable place for the deposition of their eggs, the eggs occurring in the ovarian tubes and in the oviduct differed in the following extremely interesting manner. The egg which was fixed between the vulva and the orifice of the seminal receptacle, had already begun to develop itself and contained an embryo, whilst the egg found in the oviduct above the orifice of the seminal duct, which was perfectly equal in size with the preceding one, did not betray a trace of the commencement of the development of the embryo, any more than the eggs contained in the Fallopian tubes. In such female flies the seminal receptacle always contained mobile spermatozoa. At that time we contented ourselves in the explanation of the process of fecundation, with the supposition that the contact of the spermatozooids sufficed to incite the egg to development. More recently we have been compelled to drop this theory of contact, since we have been able to trace the penetration of the spermatozooids into the interior of the egg. The process of impregnation will now have to be more precisely conceived in the following manner: The fecundation and capability of the development of the egg are not produced merely by the immediate contact of the semen with the egg, but the elementary constituents of the semen, the mobile seminal filaments must actually slip into the interior of the egg, very probably to become destroyed here first, to be dissolved and then mixed with the elementary constituents of the egg.* For this purpose the eggs of insects possess a micropylar apparatus, that is to say, one or more small apertures at one of the poles, through which the spermatozooids must get into the interior as far as the yolk of the egg, in order to complete the act of fecundation.

Leuckart was the first to set himself the task of ascertaining by direct observation, to what modifications the penetration of the spermatozooids through the micropylar apparatus of the eggs of the bee would be subjected according to Dzierzon's theory. For this purpose he went to Seebach at the end of May last year, in order to be able to make use of the most abundant selection of the necessary objects for investigation. A better opportunity for such investigations could be presented to him nowhere else than in the immediate vicinity of the grand bee-establishment at Seebach, in which, however, we must also take into account the disinterested liberality with which Herr von Berlepsch sacrificed his aparian riches for the purposes of such physiological and anatomical investigations.

Leuckart's intention had already been announced by Berlepsch in the *Bienenzzeitung*, and I was therefore extremely anxious to know what results Leuckart would obtain from these Seebach studies. These have been recently published by Leuckart in the above-mentioned journal, from which I will here communicate the most important of the results.

Leuckart was, of course, obliged in the first place to turn his particular attention to the micropylar apparatus of the eggs of bees, of which he gave the following description: "As in the eggs of most insects we distinguish in those of bees two membranes, an inner one the so-called vitelline membrane, and an outer one the eggshell or the chorion. Both membranes are extremely thin and delicate; even the outer one, which otherwise (especially in those eggs which are deposited freely) is of considerable thickness and firmness. The vitelline membrane is structureless, whilst the chorion is covered with a delicate hexagonal lattice work, as with a network, as far as the hinder (lower) flattened end of the egg which serves for its attachment. The micropylar apparatus lies at the anterior or superior pole of the egg, which is last excluded during oviposition (and afterwards contains the head of the young larva. At this point where the ridges of the chorionic network run together, we see a little fan-shaped figure (of about 1-70 millim.) with about twelve rays. The rays of which the face is composed form the optical expression for the same number of micropylar canals which run under the surface of the chorion. At their lower diverging ends these canals are open externally, whilst they open into the internal space of the eggs with their opposite extremities. The structure is exactly the same as in a number of other hymenoptera, but with this distinction, that in this case the micropylar canals are most extraordinarily thin and delicate, so that we can hardly convince ourselves with certainty of their real nature, and at the first glance might be inclined to regard the rays as ridges, such as also occur on other parts of the chorion. The canals can hardly be more than one five-thousandth part of a millimetre in diameter, though certainly still sufficient to allow the passage of a seminal filament. During the last period of its stay in the ovary, the bee's egg receives another external coating of an albuminous nature, which, indeed, is reduced to almost nothing on the anterior (superior) half, but gradually thickens posteriorly, and acquires a very considerable development at the flattened posterior (inferior) pole. This albuminous deposit serves for the attachment of the egg to the wall of the cell." Although I do not quite agree with the Leuckartian conception of the micropylar apparatus of the bee's eggs, and am especially compelled to regard the above-mentioned micropylar canals as something else, a detailed criticism of this representation of Leuckart's would lead me too far, and I therefore reserve this for another occasion, and will make use of the expression *micropylar apparatus*, without connecting therewith exactly the same idea as that which has been formed of it by Leuckart. The possibility of success in the before-mentioned artificial impregnation must depend as Leuckart has very justly observed, upon the thin albuminous coating of the eggs of bees; for as soon as this albuminous coat has dried, which will certainly be the case within a few minutes of the deposition of the eggs in the waxen cells, the orifices of the micropylar apparatus will be plastered over with it, so that the seminal filaments

*The cases of true parthenogenesis are, of course, to be understood as forming the exceptions to this rule.

will be prevented from penetrating into the interior of the egg.

Leuckart's statement, also, that it is impossible, from the external condition of the bee's egg, to arrive at any conclusion as to the sex of the bee which is to be developed in it, is important, and I can fully confirm it.

Leuckart now hoped "by the assistance of the microscope to ascertain the presence or absence of the seminal filaments upon the micropylar apparatus of freshly-deposited drone-eggs, and from this to draw a conclusion as to their fecundation or non-fecundation," as he knew "that in many cases it is not difficult to discover the seminal filaments single or in strings, sometimes even in very considerable ones, in the albuminous layer covering the micropyle in freshly laid eggs, and even to witness the act of slipping in through the micropyle." Unfortunately these hopes were not fulfilled, for Leuckart was compelled to admit that what he observed is not sufficient for the decision of the question, and only possesses some value in as far as Dzierzon's hypothesis is not directly contradicted by it.

Leuckart accounts for the failure of his design as follows: "The bee is one of those insects which, in fecundation, only deposite very few seminal filaments, perhaps in many cases only a single one, upon their eggs. Important and significant as this circumstance is for the practical breeding of bees, as only by it does it become possible that the queen, notwithstanding her immense fertility, can lay eggs for years together without exhausting the contents of her seminal receptacle—it is evident that this is equally unfavorable and unwelcome to the physiologist who is in search of these filaments. To this, we may further add that in bee's eggs the seminal filaments have not to penetrate through a thick albuminous layer before reaching the chorion, as is so frequently the case in other insects; but that they are deposited almost immediately on the micropylar apparatus, and consequently can penetrate through its canals in a very short time. Lastly, if we consider the difficulties which are thrown in the way of microscopic preparation of the bee's egg, by the great elasticity and delicate texture of the chorion, we can hardly complain of an observer if he has not arrived at any satisfactory result in this case. I admit freely that the investigation of the bee's egg has been the most difficult of all the numerous investigations of this kind which I have undertaken during the last two summers."

By an unlucky chance Leuckart, whilst in Seebach, found no opportunity of examining perfectly fresh-laid bee's eggs, as at the time when Leuckart undertook these investigations, Von Berlepsch could, by no means, bring a queen to deposite her eggs. As the result of these Seebach studies, therefore, Leuckart could only furnish the editor of the *Bienenzitung* with the following information: "But you wish them to know to what my investigations have led in general? I reply that on two occasions only I met with some undoubted seminal filaments upon the micropyle of bees' eggs. On one occasion a single filament; on the other several, four or five, (and yet I have most carefully examined more than fifty bees' eggs). On both occasions it was

upon worker-eggs that I found the seminal filaments. On drone eggs I have never been able to distinguish a single seminal filament, although I probably examined more drone-eggs than worker-eggs, and amongst these such as had been laid at the utmost a quarter of an hour previously. You see the result is doubtful. At all events it appears rather to speak in favor of than against Dzierzon. But I must repeat that this appearance is possibly deceptive. In the interest of science it would delight me exceedingly if other observers should be more fortunate than I have been in this respect."

For the satisfaction of Leuckart, I may state here that I have actually been more fortunate than him, and that I have seen what his eyes did not succeed in beholding. "Until" (so Leuckart closes his statements relating to this subject), "either by experiments or by direct observation, the strict proof is obtained that it is only the eggs of the female bees that are impregnated, the question as to the causality of sex in bees remains an open one. Theoretical and other reasons may henceforth induce us either to support or oppose Dzierzon, and we may increase the materials for the settlement of this question in an indirect way—its decision cannot possibly be brought about thereby." I have, in fact, been able to furnish by direct observation that evidence which must have been required by science as alone sufficient for the establishment of Dzierzon's theory. How far I may consider myself justified in this assertion, may be ascertained from the following statement:

Although I knew that Leuckart was about to commence the above-mentioned investigations at Seebach, I had also long before undertaken to make similar investigations in the interest of science. Without knowing that Leuckart had already carried out his Seebach studies and without being acquainted with the results which had been obtained from them, I also went to Seebach in the middle of August last year, because I was convinced that such investigations could only be undertaken by the aid of the abundant materials which would there stand at my command by the acknowledged complaisance of Herr von Berlepsch, I certainly entertained but little hope of attaining my end, as the season of the year was already too far advanced for such investigations. On the 21st of August I was received by Herr von Berlepsch with the assurance, calculated to inspire very little encouragement, that I would probably find it difficult to meet with the necessary material which I required for my investigations at so late a period of the summer, and therefore that I had little chance of solving the problem before me, especially as Leuckart had been there at Whitsuntide, and therefore at a more favorable season of the year, but had departed without attaining his object, and confessed to Von Berlepsch that questions relating to Dzierzon's theory could not be solved by the microscope, on account of the very great difficulties which were to be overcome in the investigations to be made for that purpose. Nevertheless, I did not allow myself to be deterred from entering upon these investigations.

I was, however, actually astonished at the bee material which offered itself to me in See-

bach, for the masses of bee colonies as well as their judicious arrangement, so favorable to observations of every kind, surpassed all my expectations. I found a hundred and four Dzierzon hives overflowing with honey and bees, destined for hibernation, and indeed distributed in various ways in eight places in a spacious orchard, amongst which I was particularly surprised at the pavilion containing twenty-eight bee-hives, already frequently referred to in the *Bienenzzeitung*. The distance of these eight bee establishments from each other was never more than forty feet Rhenish. Amongst these hives there were nine genuine Italian colonies of bees, the number of which might have been much greater, if, as Herr von Berlepsch stated, seventy Italian mothers had not been furnished by him to other bee-keepers, and the hives generally were considerably reduced by the various scientific experiments. What has been of particular service to Von Berlepsch in his bee-keeping, is the assistance of his servant Gunther, who being endowed with excellent talents, has been instructed by Berlepsch himself in the mystery of bee-keeping, and has approved himself in a distinguished manner.

I immediately set to work and examined a great number of female eggs, with which the great bee establishment of Herr von Berlepsch still furnished me in large quantities. It first occurred to me to make myself well acquainted with the organization of the eggs of bees, so as not to be exposed to delusions or errors subsequently in seeking for the spermatozooids. It was only after I had actually ascertained the structure of the egg envelopes, the micropylar apparatus, and the yolk, and practised myself in the preparation of the eggs of bees, that I turned my attention to the spermatozooids, by whose presence or absence the principal decision was to be given. Above all the most exact acquaintance was required with the individual ridges of the lattice work of the egg-shell, which is composed of irregular hexagons, as well as with the folds of the vitelline membrane accidentally produced during examination, so as not to confound these things with spermatozooids which had become motionless.

After I had in this way made myself sufficiently familiar with the examination of the eggs of bees, I had a comb brought to me at ten o'clock in the morning of the 22d of August, containing female eggs which had been deposited at the utmost an hour before. I might expect beforehand that no trace of spermatozooids would be recognizable externally on these eggs. I therefore directed all my attention to their contents, and hoped to discover the spermatozooids which had already penetrated through the micropyle in the interior of the eggs. I soon convinced myself that there was no possibility of discovering the delicate seminal filaments between the granulo-vesicular yolk masses. The linear object to be sought for was too subtle to be capable of discovery with certainty amongst the many mutually crossing outlines of the yolk vesicles. After various vain endeavors to render the interior of the bee's egg accessible to the inquiring eye, I came at last to the idea of employing an artifice, which I soon had acquired by practice,

and which allowed me to survey at least a portion of the inner space of the bee's eggs with great clearness and tranquility. I crushed a bee's egg quite gently with a very thin glass plate in such a manner that it was ruptured at its lower pole opposite the micropylar apparatus, and the yolk gradually flowed out at this spot, by which process a clear empty space was produced at the upper pole within the micropylar apparatus, between the egg envelopes and the yolk, which was retiring downwards. I directed my attention very particularly to this empty space, which I saw slowly produced under the microscope during the effusion of the yolk. The production of such a preparation, of course was not always successful, for sometimes the yolk flowed out of the ruptured envelopes without the production of this empty space; the yolk also sometimes remained diffused in the upper part and allowed no certain judgment as to the presence or absence of seminal filaments. An error in the crushing of the egg, a little too much pressure upon it, or perhaps also a peculiar less tenacious consistency of the yolk, probably caused the contents of the yolk to retire in every direction from the pressure, and therefore also to press upwards against the micropylar apparatus.

From the above-mentioned comb I examined ten eggs, which I succeeded in transferring, uninjured from their cells, upon an object glass, which, from the delicacy of these eggs, is always a matter of difficulty. The result of their microscopic examination was as follows:

The first female egg exhibited nothing remarkable. In the interior of the second egg to my great delight, I observed three distinct but motionless seminal filaments within the empty space which had been produced at the superior pole of the egg by the flowing out of the yolk through the inferior pole. In a third egg after the retirement of the yolk, I saw in the superior space of the egg which had become empty, a single motionless seminal filament. In a fourth egg I again observed three motionless seminal filaments at the same spot. A fifth egg prepared in the same way, exhibited no seminal filaments. A sixth and seventh had probably been too strongly squeezed in their preparation; the necessary empty space could not be produced in the interior of the egg at its superior pole, for which reason I regarded these preparations as of no use for investigation. In an eighth and ninth egg successfully prepared, I again saw a single motionless filament in the superior empty space of the cavity of the egg. In the tenth egg the preparation was quite unsuccessful. This comb, with female eggs, after being carefully preserved in a room, was made use of for the continuation of these investigations at eight o'clock in the morning of the 23d of August. An eleventh egg was spoiled during preparation, as also a twelfth. A thirteenth egg was in an extremely interesting condition. After it had been twenty-two hours out of the bee-hive and had been successfully prepared in the way above described, it exhibited two spermatozooids in the clear empty space between the egg membranes and the yolk, which had retired on the crushing of the egg. One of the seminal filaments performed very lively tortuous

movements. The second filament was rigid, but adhered firmly to the other seminal filament, and was thus moved by its movements. These movements were first seen by me at about half-past eight, and were also observed by Von Berlepsch and Gunther, and by two other witnesses. Three minutes afterwards the seminal filaments were still active. The preparation was then put by and not again examined under the microscope for fifteen minutes. The movements of the first seminal filament had then ceased also, but both spermatozoids, although motionless, were still distinguishable in the same spot. A fourteenth egg furnished no result, its preparation being unsuccessful. In a fifteenth four distinct but motionless spermatozoids were discoverable in the space which had become empty during the preparation between the envelopes and the retreating yolk.

On the same day another comb with female eggs was removed from another bee-hive; they might at the utmost have been twelve hours old. The investigations continued with these eggs gave the following results. A sixteenth egg, the preparation of which turned out well, exhibited no seminal filaments in its interior. With the seventeenth egg the preparation was unsuccessful. An eighteen egg contained three seminal filaments in the spot above mentioned; *one of these was active.* In the nineteenth and twentieth eggs the preparation was unsuccessful. The twenty-first contained two motionless seminal filaments, as did also the twenty-second. In the twenty-third egg on the contrary, I could distinguish four motionless seminal filaments. With the twenty-fourth and twenty-fifth eggs the preparation was unsuccessful. The twenty-sixth and twenty-seventh again exhibited each a single motionless filament, and the twenty-eighth, two of them. The four following eggs all showed only a single motionless seminal filament. The examination of the thirty-third egg was again unsuccessful. The thirty-fourth and thirty-fifth eggs exhibited three motionless spermatozoids, and the thirty-sixth egg examined by me contained *one active* and three motionless seminal filaments. In the thirty-seventh and thirty-eighth eggs, I could only perceive a motionless spermatozoid; in the thirty-ninth, fortieth, and forty-first on the contrary, I was able to discover two rigid spermatozoids.

On the 23d of August a third comb furnished with female eggs, was also employed for investigation, the eggs in which had only just been deposited. These eggs, however, did not show themselves favorable to the above-described mode of investigation employed by me, because the yolk would not detach itself so easily from the vitelline membrane after the rupture of the envelopes. But when I succeeded in producing the empty space between the envelopes of the egg and the yolk in these eggs, I often found it possible to discover spermatozoids in their interior. Not to weary the reader, I will only enumerate a portion of these investigations in their order: The forty-third egg allowed a motionless seminal filament to be detected, sitting externally on the micropylar apparatus. The forty-fourth and forty-fifth eggs furnished no

results from unsuccessful preparation. The examination of these was not repeated until seven o'clock in the morning of the 24th of August, when these deposited eggs were fifteen hours old. The forty-sixth egg contained several coiled but motionless spermatozoids. In the forty-seventh egg I was able to discover one motionless seminal filament. With the forty-eighth the preparation was unsuccessful, and with the forty-ninth and fiftieth, I was obliged to leave it doubtful whether the object which might have been taken for a seminal filament, was such in reality. Both the fifty-first and fifty-second eggs allowed a motionless seminal filament to be clearly distinguished in the empty space, when the yolk had retracted itself downward from the micropylar apparatus by the rupture of the egg shells.

If I sum up the observations just referred to, they furnish on the whole a very favorable result, considering the difficulties of the investigation, for I have also convinced myself that these investigations of the egg of the bee are, as Leuckart has very justly asserted, amongst the most difficult of all investigations of the kind. Amongst the fifty-two female bee-eggs examined by me with the greatest care and conscientiously, thirty furnished a positive result; that is to say, in thirty I could prove the existence of seminal filaments, in which movements could be detected in three eggs. Of the other twenty-two eggs, twelve were unsuccessful in their preparation. At the same time I may also indicate particularly that the observations with positive and negative results followed each other quite irregularly, but alternating at very short intervals, which probably was only dependent upon the favorable or unfavorable consequences of my preparation of the eggs employed for observation. If the question is to be raised why Leuckart was not so fortunate as to see what I have succeeded in seeing, I can make no other answer, but that probably the different mode followed by us in our investigation is to be blamed for Leuckart's want of success. Berlepsch informed me that Leuckart did not examine the contents of the eggs by the careful compression of the bee's egg, but that he confined himself to submitting the eggs in a perfectly uninjured state to an external examination.

It is certainly to my mode of investigation alone that I am indebted for the successful result of these observations, which were made with an excellent microscope of Kellner's. The careful rupture of the egg membrane effected always by me, must prove an extremely important manipulation, for by this alone it was possible evidently to isolate the delicate seminal filaments which had penetrated into the cavity of the eggs and become concealed by the yolk mass, as after penetrating into the egg they probably continue adhering for some time to the micropylar apparatus by their caudal extremity and remain behind, isolated in the upper empty portion of the cavity of the egg during the issue of the yolk mass after the rupture of the membranes.

Above all things, however, it was of consequence to me that I should be able to examine

male eggs (drone eggs) also in exactly the same way; and Herr von Berlepsch actually procured for me the means of doing this, although at first he had given me but little hope of obtaining such drone eggs even in small numbers. It was truly a *chef-d'œuvre* to obtain drone eggs at so late a season; how my acute and experienced friend, as it were, compelled a queen to lay male eggs, the reader will be able to understand from what follows.

In No. 79 of his bee hives, Herr von Berlepsch possessed a queen, which he knew to be near her death, as her bees had been constantly constructing royal cells since the end of June, and the queen furnished these with eggs by which her loss might be replaced. Berlepsch, however, had not permitted the larvae in these cells to come to exclusion, and thus this aged mother was still alive when I arrived at Seebach and inquired for drone eggs. A little while before this queen had laid drone eggs, but Berlepsch had destroyed this drone-brood also as being useless. At last the workers had enough of it, and commenced no more cells. Berlepsch's object in this case was to determine how long the life of a queen might be prolonged artificially. When I came to Seebach this queen was still laying an occasional egg. On the 21st of August Gunther received the charge to feed the hive, No. 79, in the evening with fluid honey. The next evening (August 23d) two combs with covered worker-brood and between the two an empty drone-comb, were suspended in this hive. The following morning (August 23d) there were twenty-seven drone-eggs in this drone-comb, and about sixty worker eggs in open cells of the worker combs. Berlepsch had carefully ascertained previously that not a single egg was present in the open cells of both the foreign worker-combs when suspended in the experimental hive.

I examined these twenty-seven drone-eggs which might have been about twelve hours old, and which agreed perfectly both in their appearance and organization with the female eggs, with the same care and by the same method with which I had treated the female eggs, and did not find one seminal filament in any one of the eggs, either externally or internally. I must also add that only the seventh, thirteenth, and twenty-third eggs were unsuccessfully prepared. In all the rest of these drone-eggs the yolk retreated slowly and completely from the upper pole of the egg-envelopes, after bursting the membranes; the desired empty clear space between the micropylar apparatus and the retreating yolk was produced in the interior of these eggs, so that if seminal filaments had been present in them, they certainly would not have escaped my searching and inquisitive eye. In order to be quite satisfied as to this remarkable negative result, and to obtain the full signification of it, several female eggs of the same queen which had furnished these drone eggs were examined for comparison; for the objection might certainly have been raised, that this queen might have laid nothing but barren eggs, as being already weakened by age and near her death, she might have had no more spermatozooids in her seminal receptacle. Nevertheless,

many of these eggs contained seminal filaments; they were the twenty-seven eggs already mentioned by me, namely: the sixteenth to the forty-second eggs.

To this result of my Seebach investigations, which proves the correctness of Dzierzon's theory by direct observations, I may also add that Herr von Berlepsch has lately informed me by letter, that this queen subsequently after my departure from Seebach, also laid female eggs, from which workers were developed; but she herself only died on the 19th of September, 1855.

[For the American Bee Journal.]

Italian Queens.

MR. EDITOR:—Allow me to give you a few jottings of my experience in apiculture.

I commenced in a small way a number of years ago, to keep a few stocks of bees. At that time it was but little trouble to take care of them, as they almost invariably did well in any kind of hive or gum; for the country was then new and wild flowers abundant. But soon there was trouble in the camp. The bee-moth began its ravages among the stocks, and in a short time my prosperous colonies dwindled away; and it was only by the greatest care that I managed to keep my stocks from being entirely destroyed. Thus bee culture became neglected, as it was a source of vexation and no profit.

Something had to be done, and we are indebted to the practical investigations of Huber, Dzierzon, and others in Europe, and of the Rev. L. L. Langstroth and others in America; and their improvements in moveable comb hives have proved that success in bee culture does not depend upon luck, but upon proper management. And now the careful apiarian can increase his colonies at pleasure, and in ordinary seasons look for a good supply of surplus honey to pay him for his labor.

Last spring being convinced that the Italian bees were more prolific and more easily managed than the black bees, I commenced to Italianize, and had the pleasure to see at the end of the season that I had increased my stock to twice the number, all in good condition for going into winter quarters. I purchased my Italian queens from Mr. Adam Grimm, of Jefferson, Wisconsin, and they proved to be just as he represented them, very fine. Mr. Grimm wrote to me since his return from Italy, where he had been to purchase a lot of Italian queens of Prof. Mona; and as I wanted to see queens just from their native Italy, I concluded to make Mr. Grimm a visit. I went and found him busy in preparing his extensive apiary for the introduction of the queens just imported by him. I had the pleasure of seeing all his imported queens, and found them invariably dark-colored; (with two or three exceptions) yet they showed distinctly the yellow stripes peculiar to the Italians, and the workers accompanying them were beautifully marked. Mr. Grimm's apiary is perhaps the largest in the West. At this time he has six hundred and six colonies. His home apiary numbers over three

hundred colonies. The other apiary is two miles and a half distant. All the colonies seemed to be in good wintering condition. Mr. G. uses the Langstroth hive. I am satisfied that if there are any pure Italian bees in this country, Mr. Grimm has them; and if Mr. Victim and others who are so afraid of being victimized would purchase a queen of Mr. G., with his guarantee of purity, I would wager a red apple that he or they would get what they want. I stayed with Mr. G. two days and left for home, taking with me several of his beautiful queens for my own apiary in Ohio.

S. SANFORD.

LIMA, OHIO, Oct. 29, 1867.

Kentucky Bee-Keepers' Association.

LEXINGTON, KY., Nov. 25, 1867.

EDITOR BEE JOURNAL:—Pursuant to a call issued some time ago, quite a number of bee-keepers met at the Court-house, in this city, on the 20th instant, and organized a KENTUCKY BEE KEEPERS' ASSOCIATION. Inclosed you will find an account of the proceedings.

D. BURBANK.

MEETING OF BEE KEEPERS.

LEXINGTON, Nov. 20, 1867.

Pursuant to notice previously given through the papers, quite a large number of bee keepers from various parts of the State met at the Court-house in this city, and on motion, R. T. Dillard, D. D., was called to the Chair, and J. W. Reynolds appointed Secretary.

Mr. D. Burbank, in a few pertinent remarks, stated the object of the meeting to be for the purpose of organizing an association of all interested in bee culture, and consulting as to the best means of encouraging and advancing this important interest.

Mr. Burbank moved the appointment of a committee to arrange business for the consideration of the convention, which motion was adopted.

Dr. John Dillard, Messrs. R. A. Broadhurst, Wade, Egbert, Burbank, and Brown were appointed said committee.

R. A. Broadhurst, Principal of the Female Orphan School at Midway, addressed the convention on the importance and profitableness of bee keeping, fortifying his conclusions by examples under his own experience; demonstrating beyond doubt that a farmer cannot lay out the small capital required to commence the business in any manner to secure as certain and lucrative return. He described his apiary on the plan he pursued, and imparted a great amount of valuable information concerning the subject.

We do not pretend to give anything like a full and accurate report of the speaker's instructive and entertaining remarks. From his experience with the common and Italian bees, he regarded the latter as most profitable.

Dr. John Dillard said he commenced keeping bees about five years ago, with nine swarms of bees, using the common hives. By a short experience, after losing numerous swarms, he adopted the Langstroth hive, and he is con-

vinced that it is the hive best adapted for bee keepers here and elsewhere. He has carefully observed the Italian bees, their habits, &c., and is fully satisfied that they are by far the most profitable. They commence working earlier in the season and continue later. He has observed that they work better on the second crop of red clover than the common bees.

Capt. N. Craig, of Scott, addressed the meeting at length. He has been a practical bee keeper for fifty years, and approves of Langstroth's system of bee-keeping; thinks the Italian the most profitable bee. It is a longer lived and a better worker. He thinks yellow pine the best material for making hives; thinks the frames in the interior should be of white pine or yellow poplar. In regard to moths, the best remedy is cleanliness of hives. He prefers the Langstroth movable comb hive to any other that he has used! Thinks buckwheat good bee pasture.

The Rev. Dr. Dillard made some remarks as to the pleasure and profits of bee-keeping. Has been a bee-keeper for fifty years. Thinks Langstroth's the best hive. Said persons in the highest ranks in Prussia and other countries in Europe devote much attention to bee-keeping.

The committee to arrange business for the convention reported the following officers for a permanent organization of a Kentucky Bee Keepers' Association: Dr. John Dillard, of Fayette, President; R. A. Broadhurst, of Midway, S. F. Drane, of Eminence, William J. Moore, of Danville, N. A. Rapier, of New Haven, D. L. Adair, of Hawesville, and Cyrus Simmons, of Bowling Green, Vice Presidents; Wesley Spencer, Secretary, and D. Burbank, Treasurer.

The committee also reported various subjects for discussion, only one of which was considered for want of time, viz: the best mode of wintering bees.

Dr. John Dillard gave his experience; was of opinion that the most important matter was to prevent dampness, has used straw in the tops of his hives to absorb the moisture from the respiration of the bees, and keep them warm, but thinks corn cobs, being non-conductors of heat and great absorbants of moisture, the best thing he has tried; he removes the honey boards and caps, and packs the vacant space above the frames with cobs.

S. C. Brown corroborated the remarks of Dr. Dillard, and spoke at length upon the advantages and disadvantages of wintering bees in dark cellars or buildings, or burying them, &c.

David Butcher preferred wintering bees on their summer stands. His plan is to place over the hives large boxes and fill the space between the hives and the inside of the boxes with saw dust.

Dr. George S. Savage moved that when this meeting adjourn, it be to meet at this place on the second Tuesday of December, at 11 o'clock A. M. Agreed to.

D. Burbank moved the appointment of a committee to draft a constitution for the association and report at the next meeting. Adopted.

The chair appointed as the committee D. Bur-

bank, J. M. Tipton, Dr. John Dillard, and Dr. R. J. Spurr.

Dr. Savage moved the appointment of a committee to procure a speaker to address the association at the next meeting. Adopted.

D. Burbank and R. A. Broadhurst were appointed said committee.

The convention then adjourned.

J. W. REYNOLDS, Secretary.

[For the American Bee Journal.]
Italian Bee-Breeding.

What is wanted is light color in all the bees, strong constitutions, good workers, not disposed to sting if well managed.

To obtain light colors, raise from the lightest colored parents of both sides; avoid breeding in-and-in further than to get the color, as it has a tendency to weakness if carried too far.

To improve the constitution and workers, breed from the best not near related.

To improve the disposition, breed from parents that show these qualities most. This object can be arrived at more speedily by breeding in-and-in, as it will take only a few generations to make them too docile to defend themselves at all. Therefore if carried too far in that direction, like an overgrown branch of a tree, it fails the first ill-wind.

To succeed, have control over the hive, bees, and combs. Let such queens as are selected lay in the drone-combs, and no others be allowed to produce drones at all; and the drone mothers no queens, unless the object is to breed in-and-in—which is a poor plan if the desired qualities can be got without.

The breeding should be done generally by one pair of queens for an entire apiary each year. Many generations of queens can be raised after all other drones are killed, if the hives containing the drones wanted are kept queenless.

JAMES M. MARVIN.

ST. CHARLES, ILL.

[For the American Bee Journal.]
Regicidal Attacks.

In rearing queens last summer, at first one-third of the number were lost by regicidal attacks. This was more frequently the case in nuclei containing young bees exclusively. Returning from her wedding flight, the queen has acquired a peculiar odor, and is hence not readily recognized by her companions, but received and treated as a stranger—being enclosed by angry bees in a ball or cluster about the size of a hen's egg. A whiff of smoke will scatter these bees. The bees composing this angry ball will sting each other, and are dragged out in front of the hive. This gives the careful aparian notice of what is going on within. Caging the queen over night within the hive is necessary to insure her safety.

DELHI.

DAVIS.

Send us names of bee-keepers with their post office address.

[For the American Bee Journal.]

Questions by Querist.—No. 3.

Perhaps some of the JOURNAL readers have been looking for more questions by Querist. If so, they shall be gratified. Querist is highly pleased with many of the answers to his questions in No. 2, and hopes those in this number will be as promptly attended to.

The most of us are satisfied that *frame hives* are better than *box hives* for bee-keepers in general; and as that point seems to be pretty well settled, let us now settle two more points, viz: the *best size* and the *best form* for hives.

These two features are inherent in *all* bee hives and are not covered by a *patent*, nor can they be, for they have been public property as long at least as bees have been domesticated. I hope, therefore, that no writer who proposes to discuss the points named will make use of the word *patent*, for the purpose of bringing before our notice some particular patent hive that may be worthless or otherwise.

Those who have given these points—*size* and *form*—careful thought, will find that each of them may require an article by itself, and that it will then be as long as most of us are willing to read. Some of the JOURNAL readers may think that Quinby, Langstroth, and others, have given us the *best size* for a hive. It may be so, but I have my doubts about it. Two thousand cubic inches, or about a bushel, may be as they claim the *best size*; but if so, let us have the reasons why.

I presume no one will claim that the *best shape* has been settled among all classes of bee-keepers, for if this were true, we would not find so many forms of hives in use. Now there must be some particular shape that will secure the bee-keeper more advantages than any other. It may be necessary to discuss the *best shape* for general purposes—that is, for those who winter their bees in the open air; and also the *best shape* of hive for those who winter in suitable repositories. Latitude may also have something to do with the shape of hives, as bees south of latitude 38° would winter in the open air in hives of less depth than several degrees north of it.

QUERIST.

[For the American Bee Journal.]

Straight Combs.

I use dividing boards with a few holes bored through them. They are placed in the hive the same as the frames; first a frame and then a board. Put pieces of combs on the frames, fastened with melted beeswax on the underside of the frame. The bees will lengthen them perfectly straight.

DELHI.

DAVIS.

There is a species of butterfly found at the Cape of Good Hope which, when seized or attacked, defends itself like the honey-bee by stinging. It is the only one of that class of insects known to be provided with a sting. It is there called the *bee-moth*.

[For the Bee Journal.]

How about Pure Italians?

DEAR JOURNAL:—I want some light on the Italian bee question. I have purchased two Italian queens, and have raised a dozen. My bees now are all Italians and hybrids, or they are all hybrids. How am I to know which? Only about five out of every six of the workers from the purchased queens show the three yellow bands; the other one-sixth show only two. On the other hand, the workers from the queens which I raised, vary greatly. In most of the hives, about one-sixth of them show the three bands, and the other five-sixths show only two distinctly. As to temper, they are all about alike—a little more amiable than the black bees. I found, when nearly the whole colony was hybrid, that still the black bees annoyed me more than the hybrids; yet my bees will all sting.

Cannot some one who has eyes, who can count a hundred, and who has not made up his mind on the distinguishing marks of Italians, visit some apiary where imported queens are kept, and examine carefully a large number of workers, both of imported queens and of American born queens, and report the result to the Journal. There is truth in the matter, if we can only get it. One misfortune is that the observations of many persons are worthless. You cannot trust their eyes, and must receive with allowance their honest statements. I lately called to see a man who has long kept Italian bees. I wanted light. He informed me that there was no doubt about all pure Italians showing the three bands. I went to see. Putting his hand on a hive he said, "here is a queen that I am very sure produces pure workers." I stooped down and commenced examining carefully the bees at the entrance. I saw at once that about one-fourth of them showed only two yellow bands. I told him so. He would not believe. I pointed out the bees. He caught one to pull its abdomen and show me that *the yellow was there, but covered up under one of the folds.* It was hard to find. But still he is confident that all pure Italians *show distinctly* the three yellow bands; and he is equally confident that he has pure Italians. I believe he writes sometimes for the Journal. There is no use in such persons as my friend going to see other people's bees. They know, without looking, that all Italians show the three marks. I hope they do, and that the question will soon be settled.

Many persons have the opportunity to examine the workers produced from imported queens. There can be no doubt about their purity. Will some one have the goodness to make the examination, and report *the facts* in the Journal. Let him tell us how many bees he has examined, and how many show distinctly the three yellow bands. And, above all, let him keep his eyes open for those which show but two bands. I have a notion that even if there be such among pure Italians, they are harder to see than those which have the three bands. Indeed it is quite probable that some

people could not see them at all. But I want light. Who will give it?

INQUIRER.

[For the American Bee Journal.]

Experience of a Novice.—No. 10.

DEAR BEE JOURNAL:—The following items of experience may be acceptable:

During the dry weather the past fall there was considerable robbing among the bees in our vicinity.

About the middle of August, I observed one evening two swarms of black bees that I had bought this season to be very busy till nearly dark. Supposing that some other bees were robbing them, I proceeded to contract the entrance; but soon saw that the returning bees, instead of being light, were so heavy that in many cases they fell to the ground before reaching the hive, and on killing a few found them gorged with honey. Next morning I found them again at work, and finally lined them to a neighbor's, where one of his best hives, as he called it, was literally black with bees, which we eventually found coming from four different apiaries. Of course the honey was soon gone; but the remarkable part of it was that not an Italian bee was seen there, although I had twenty-three Italian stocks, and only the two black ones. The Italians were at the time at work on red clover, and on nothing else that we could discover.

Mr. Editor, do you not think it rather harsh and ungentlemanly for a correspondent to make the assertion that he does not believe any one ever had seen Italian bees work freely on red clover, after the contributions we have had on the subject? Why should beekeepers use so little forbearance with each other? It seems to me that we can develop real facts from actual experience, without so much picking at each other and doubting each other's statements. For instance, Professor Varro reasoning to show that Mr. Quinby *could not* have had a fertile queen smaller than a worker. Now, Mr. Editor, I did raise a queen *much smaller than an ordinary worker.* A number of my friends saw her, and laughingly called her my "Bantam queen." Contrary to our expectation, she met a drone and was seen to return with the usual drone appendage. She afterwards laid eggs which produced fine Italian workers; but she laid so few that, after keeping her about three months, she was replaced by a more fertile queen. We do not think her eggs produced any drones at all. She was raised in cool weather, in a small nucleus, and was nearly all of a jet black, although raised from my first Langstroth queen.

To return to the robbing. The Italians *did* learn it later in the fall. After buckwheat was gone, all queenless hives, for a mile or more around, were greedily appropriated; and the number was quite considerable, so that one stock of hybrids worked some in boxes on stolen honey. This gave the Italians rather a bad name among some; but in no case could I learn that there was any fighting, and most of

the hives despoiled were already occupied by the moth worm.

On one occasion I bought a box hive of black bees that would make no attempt to protect their stores; and after trying every way to stir up their anger and make them show fight, in despair I shook about a teacupful of young Italians into the hive one evening, and by contracting the entrance these few young bees next morning repelled a host of black robbers that had got well started too, as they had previously carried off nearly half of the contents.

I have no fear of Italians being robbed, whether queenless or not.

As an experiment I put two black queens in one hive, and after half an hour found both well treated, as the stranger had been from the first; but they were on separate frames. In fact I do not know whether I removed the one I put in, or the original. Could not queens be introduced in that way, without the bees being conscious of any loss at all?

A second experiment of the same kind resulted in finding one queen dead at the entrance of the hive after an hour's time. I could not tell which one, as I had not marked the stranger.

At the third experiment, I found the two queens both well treated, on the same side of the same comb, after about an hour. The bees were working but very little at the time.

One more item and I am through.

As I have been at considerable expense in trying both the Langstroth and the American hive, I was deeply interested in two articles on hives in the December number of the BEE JOURNAL, and must say the ideas given me in them are worth more than all I have paid for the JOURNAL many times over. The first year the bees were in the American hive, I had but little trouble in opening them. But a few days ago, in trying to open some of my heavy stocks in cool weather, I found much trouble in getting the movable side out, and more in getting the frames apart, and finally so exasperated the bees in closing them that they came up to the top of the frames, so that I had to resort to smoke—something I very seldom do. Again, in attempting to exchange some thick heavy combs for light thin ones, I was obliged to give it up on account of the frames being kept at equal distances.

If I succeed in wintering in the Langstroth hive this winter, as well as the American hive, I think I shall conclude that Mr. Langstroth knew what he was doing when he made his hives the shape he did.

I certainly have had more surplus honey from the Langstroth hives this year than from the American, but have been trying in vain to find some other reason for it. I used the shallow boxes made for the American hive, put them directly on the frames, and put an empty one under when half full. I have had no trouble with brood in the boxes, but in some cases some pollen.

This winter I have made an inch and a half hole in the end of the Langstroth caps, covered with wire cloth, and then filled the cap closely with straw, replacing the whole after removing

the honey-board. I always winter in the open air. At present—December 10—they are dry and healthy.

An earnest desire for improvement, and good will among all beekeepers, is the prayer of

NOVICE.

[For the American Bee Journal.]

Proposals Wanted.

In the October number of the JOURNAL, page 73, we find an article under the above heading. Well, friend Marvin, we will propose.

First. If we understand you, you intimate that hybrids are better workers than pure Italians. If so, why not be satisfied with them? Or if not in possession of them, why not wish proposals for them?

Secondly. We can furnish you with such a queen as will reproduce others like herself to an indefinite number, and all counterparts of herself, particularly if all made in one mould.

Thirdly. A queen as yellow as beaten gold, and which will reproduce others in any number, of course for the same price.

Fourthly. As to the prolificness of this queen we cannot speak. That would perhaps depend on the purse of the purchaser. As to the workers and drones they would certainly be yellow, like the queen, unless we should fancy to have their feathers dyed. As to their docility, we can speak in the highest terms; but of their locomotive powers we cannot say much. The price will certainly be high; and why should it not be for such extra stock? We cannot state the exact sum until we consult a goldsmith.

But, seriously, with articles like that referred to, breeders of Italian queens have much trouble. They have a tendency to produce doubt as to the purity of Italian bees, in the minds of many who contemplate introducing the Italian variety in their apiaries. Why not be satisfied with the Italians as we have them? Yea, we should be grateful for a kind of bees that so much interest us, and add so largely charms of apriarian knowledge; and not only this, but adds at the same time to our pecuniary resources.

Again we say, why inquire for a thing that has no existence? Perhaps, indeed, the article was intended for such persons as come before us boasting great things and displaying cuts, &c. If so, we say amen! but it should have been intimated.

R. B. OLDT.

NEW BERLIN, PA.

Where it is intended to perform an operation on a populous colony, which is likely to require much time, it should be undertaken about noon on a clear and calm day, when many of the bees are abroad. The combs are then less crowded, and the honey-laden bees returning from their foraging excursions, are less irritable than under other circumstances. A still better plan is to remove the hive to some distance and set an empty or decoy hive in its place on the stand, till the intended operation has been performed.

Send us names of bee-keepers with their post office address.

[For the American Bee Journal]
Various Items.

EDITOR BEE JOURNAL:—After sending you my letter of September 12th, from Bellinzona, Canton Tessin, I visited a great number of apiaries in the neighborhood of Bellinzona, where Prof. Mona bought swarms to get the queens he was constantly sending off. I had thus an opportunity to see and compare about thirty more queens, besides the hundred which the Professor had packed or was packing for me. I am positive that there were not two of them exactly alike in color and size. I have seen all shades of color, from one whose abdomen was brown-yellow or leather colored, except the point, down to perfect black ones. The large-sized queens were usually handsomer than the smaller ones, which were either perfectly black, or had but very narrow yellow rings, where the workers have their three rings. The yellow rings were narrower on these queens, than on their worker progeny. With all this difference of color and size in queens, I could not find a perceptible difference as far as the workers are concerned. When I supposed I had found a difference, and examined the swarm more closely, I could quickly satisfy myself that the workers I saw at the entrance of the hive were merely older bees than those I saw in front of neighboring hives. Being fully satisfied that, at least as far as I went, no common or black bees could be found, I concluded to examine drones only, thereafter. All the drones I was able to find at that time, (Sept. 16—18) were of course old ones, and I could not perceive the least difference. They seemed to me to be smaller than the common black drones, and differed so little in color from each other, that an inexperienced observer would have mistaken them for black drones. All the difference consisted in three very narrow stripes on the edge of the first three rings of the abdomen, where the worker has the black edge to the yellow bands. By very close examination, however, I found some brown spots on those parts of the first three rings, where the workers have the yellow bands. Mr. Uhle told me that these spots are more visible when the drones are young.

Comparing my own former view with the statements of Mr. Dzierzon (who, by the way, does not claim that he ever saw Italy, or the bees there), that the Italian bee in Italy itself is not perfectly pure, with my present observations, I must concede that I was mistaken. I am now fully satisfied that the Italian bee, at least in the neighborhood where I have examined it, is a pure distinct breed—distinct from the black bee, however much the queens may differ in color and size. If this were not so, how could the workers of a perfectly black queen (one of those which I showed to Dr. S. Sanford, of Lima, Ohio,) be exactly of the same color as those of the brightest queen? Much rather do I now believe that the golden-colored queens are in some way bastardized. Dr. Ziwanski writes, in his annual report to the German Bee-Keepers' Convention, for 1865, "Whoever ex-

pects an original Italian queen light or yellow colored, deceives himself. They are all of them more dark than light, almost black, and one who does not know them, would find it difficult to discover any difference, nay, would perhaps find a common queen lighter colored than an Italian. The lighter and the more splendidly yellow an Italian queen looks (those from Pollegio never look so), the greater ground there is for suspicion." All the daughters of originally imported queens are dark again, but produce the prettiest workers. Mr. Dathe, who, however, never saw Italy nor the bees there, agrees with Dzierzon, who considers only those queens pure that are perfectly yellow, and then says, on the next page of his pamphlet, "When we declare the pretty yellow colored Italian race as the pure full-blood race, we do not mean to say that the darker or browner colored race is less valuable in a practical view. We find everywhere in nature the lighter color more noble and tender, the darker more durable. *So are the darker queens less tender.*" Travelers in that portion of the country where the striped Ligurian bee is found, reported in former years in the German *Bienenzitung*, that the nicest colored bees were found in the neighborhood of Lago Maggiore. Shall we then take the Italian bees found in this part of Italy as the standard of Italian bees? Or shall we accept as the standard Dzierzon's bees, who succeeded in propagating higher colored queens from the original imported stock? As I have more than forty queens which I imported from Italy, safely introduced in my apiary, I shall have the means of making more careful examinations next summer. If I do not lose them during the winter, I will in due time send further reports.

PROF. MONA'S EXPERIENCE ON NICE QUEENS.

Conversing with Prof. Mona, one day, about the beautiful golden yellow colored queens sold in America, he related the following: "We had a pretty nice queen this spring, and intended to breed from her, as our customers in Germany often ordered nice colored queens; but we had so many crippled and deficient queens from her progeny, a number of which never laid an egg, and also three whose eggs never hatched, that we rejected her as unfit to breed from. We are convinced that the darker queens are harder and more prolific." I must add that on my return from Europe, I found in my apiary four of those pretty yellow queens whose eggs would never hatch. One of them was destroyed in the presence of Mr. Crowfoot, of Hartford, Wisconsin.

A COLONY OF ITALIAN BEES IN A VERY SMALL HIVE.

On the 13th of July, 1865, I hived a small second swarm in a hive eight inches high, ten inches wide, and sixteen inches long, containing eleven frames six by eight inches in the clear. This little second swarm filled its hive with combs, and stored honey enough to winter on. In the spring of 1866, it was so weak that it could brood only on one comb during the month of April. It however multiplied so rapidly afterward, that it filled two top boxes with honey weighing twenty-four pounds, and had

honey enough in the hive to winter on. This spring it came out all right. I made an artificial swarm from it which filled a double hive of the size of the mother hive, and stored about ten pounds of honey in boxes. The mother stock filled its hive, and made about fifteen pounds surplus honey. I have not noticed a drone in this little hive for three seasons.

HOW TO SEPARATE SWARMS THAT MIX DURING SWARMING.

If two or more swarms unite during swarming, I do not attempt to separate them immediately. I hive the whole together in a large eleven frame hive, place them in a cellar for half an hour, light a candle, then take out about half of the frames with the bees on into another empty hive. I then look for the queens, which are usually found in a small cluster of bees, on the bottom of the hive. Lifting this cluster out with a spoon, I part them with some tobacco smoke, catch and cage the queens, suspend one between the frames of each hive, divide the bees equally, and let them stand in the cellar over night. A. GRIMM.

JEFFERSON, WIS., Nov. 6, 1867.

[For the American Bee Journal.]

Common Failures.

MR. EDITOR:—Those Bee Journals came promptly. We owe its correspondents and editor a debt of gratitude for their philanthropic and gentlemanly course in diffusing knowledge upon the interesting and neglected subject of bee-culture.

In canvassing several towns of this State I found, with the exception of one or two in each town, no scientific bee keepers. Many have been induced by local agents to buy comb frame hives and transfer their bees even as late as August. And as several of our last honey seasons, especially 1866, were very unfavorable, most of the transferred stock perished, which was generally attributed to the hives. In answer to inquiries, I have heard the following experience from different individuals, whom, for convenience sake, we will suppose to be one man:

"My father before me always kept bees, and I have kept them ever since; and we know how to keep them by this time, you see. But now the patent hives, cold winters, millers, mould, and robbers, have ruined my bees."

How so?

"At one time we got up to twenty swarms, all in box hives (the best hive in the world.) The next winter the snow drifted over them some, and one-half of them died, with honey enough in the combs, and to spare. Some time in the spring, several more were robbed, honey, bees, and all. Then my bees did not swarm as usual that season, except one that swarmed three times, and went back every time. I thought the millers troubled them, so I lifted them up and put some cobs under, that the worms could not get up; but for all that they destroyed a number of swarms. The next winter I put them in my chamber, and during a pleasant day they some way got the rags and

papers out of the holes, and at night they were all over the windows and floors, dying. The remainder did better that year. The next winter I stopped them up with cobs to keep the mice out, giving them air enough as I supposed, and put them into the cellar as neighbor B—did early. By the last of February, I was working at my potatoes, and something smelt bad, and grew worse every day. Finally, I looked at my bees. I found two colonies were still alive, but the mice had gnawed the cobs out and eaten the heads off of most of the bees. In the other hives the bees were at the bottom, dead, rotten, and mouldy. So you see how difficult it is to keep bees in our days."

Now there is no fiction about this. It is a fair representation of the sentiments of a majority of the beekeepers in the country, very many of whom, though good citizens and intelligent men in other respects, are totally ignorant of first principles, a knowledge of which is indispensable to profitable beekeeping. What we want, then, is to introduce the BEE JOURNAL in every beekeeper's family. This would create an interest in the subject; and then, with the aid of movable frame hives, the business can be reduced to a system and made profitable, instead of depending on luck, as many beekeepers imagine.

VERMONT.

O. C. W.

[For the American Bee Journal.]

Question.

It has been said that bees will not build combs on a painted surface. If so, its importance is obvious in many ways. Can any one give his experience in this matter?

APIS.

California.

BEES.—From the San Bernardino *Guardian* we learn that parties who go bee and honey hunting have met with a great deal of success this summer, the wild bees being very plentiful in the mountains and canons. Some of the bee trees that have been cut, yielded as high as two hundred and fifty pounds. As a general thing the bee hunters have brought in but few swarms, preferring to take their honey and let the bees remain until their hive is again full. So successful have been the culturists that there has been exported this year a quantity of honey, pure, strained honey—not in the comb, but in cans, amounting to the astonishing figures of twenty thousand pounds!—ten tons of honey, sent by a few persons from this valley this year. With such a result from such a trivial source, what would our valley not produce in the more important branches of husbandry, if proper energy and enterprise were exerted? A few years ago there was not a hive in the valley.

There is a species of parasitic larva called *Volucella bombylans*, which live in the nests of humble-bees, braving the fury of their stings and devouring their young.

[For the American Bee Journal.]

Purity of Italian Bees.

EDITOR BEE JOURNAL:—A few days ago I received the December number of the JOURNAL, and was much pleased to find that men like Prof. Varro take the trouble of publicly censuring my report about native pure Italian bees, contained in my letter addressed to you from Bellinzona. Now I wish that the Professor, as well as the other kind readers of the JOURNAL, would re-read the article on purity of Italian bees in page 19, Vol. 3, of the BEE JOURNAL, (line six from the top), where he speaks of Italian bees in their *native purity*. If he says, speaking of native pure queens, that they are brown with a black dot or two upon their body; and speaking of an exclusive reliable test of purity in Italian queens, that impeccability of temper in their worker progeny, in addition to their distinctive three yellow bands, by which they are known in Italy and Switzerland, constitutes that test; and that *he has bees that will not sting, even if the frames in their hives are smashed down*, he surely defines pure Italian queens and bees that are more than *native pure*. As he tells such a straightforward story, and his testimony is nowhere impeached, I am forced to believe him, right or wrong. But I am at a loss to know what good result bee-keepers would derive from such a test of purity, so long as no breeder of Italian bees ever claimed that he had any or has advertised any such queens or bees for sale, while the Professor refuses to sell any at any price that might be named. But the Professor changes his ground in his article in the BEE JOURNAL, Vol. 3, page 116, column 2—speaking there of a fragmental dash of impurity in native Italian bees, claiming that some black bees there, as well as here and elsewhere, do make their appearance simultaneously with three-banded, two-banded and one-banded bees, in one and the same hive; and calling in to his assistance the testimony of a gentleman he has been acquainted from his youth. Now I wish the learned Professor would once more read my report, to see whether I had noticed any of these black bees and my opinion about them—beginning at page 95, column 1, line 23 from the bottom. Those bees which a superficial or inexperienced observer would have taken for common black bees, are by no means of the common black breed of bees. On careful examination they show three *brown* bands, instead of the yellow ones of the other bees. Two days after sending off my letter from Bellinzona, I found a colony, one half of whose workers were thus brown banded, but they were not black bees; and I must further add that those bees were not all old ones, as I previously supposed, but young ones just hatched, intermingled with old ones; nor was this surprising to me. I had noticed, during my crossing the Alps, a different breed of sheep from any that I had seen in my neighborhood in America, (I am no sheep breeder), and noticed in some large flocks of white several brown sheep. Shall we, for this reason, conclude that this breed is not pure, since it is not constant in color?

When I went to Italy, I had the impression that I would find there at least a few hives of the common variety of bees, but I did not, though visiting about twenty-five different aparies. After all, I would not assume the responsibility to say that no black bees, whole colonies or single bees, are to be found in all Italy. But I was satisfied in my mind that Mona's bees answered the definition of pure Italian bees laid down by most of the German and by one English writer on Italian bees. For instance, Rev. Mr. Dzierzon answered my question put to him in a letter, regarding what constitutes the principal points of superiority in Italian bees, thus: "They are more docile and more industrious than the black bees. They defend themselves better against robbers." Rev. Mr. Kleine, on page 199 of this book on bee culture, second edition, 1864, says: "The first three rings of the worker bees are yellow-colored, but black edged. While yet young their color is lighter, but grows darker with advancing age. The drones are darker-ringed than the workers, though not differing in size from German drones. The queens are sometimes lighter, sometimes darker-colored. The lighter golden colored they are, the higher they are esteemed." Dathé in his pamphlet formerly referred to, says: "Of the six abdominal rings of the worker bees, the first two are orange yellow-colored; the third one is, according to greater or less degree of purity, more or less orange-colored or whitish; the next two rings are whitish, the edgings as well as the point of the abdomen are black. The drones partly have very narrow yellow rings, and are not unlike the German drones; partly they have a great deal of yellow." Describing the points of superiority, he says: "The Italian differs from the common bee in less irascibility, and though she can sting as well as the German bee, she is far milder, not only when not disturbed, but also during ordinary operations." Mr. Neighbor in his work "The Apiary," London, 1866, in the chapter on Ligurian on Italian bees, page 200, he says: "Their special advantages are greater fecundity of the queens, less irascibility, and a more handsome appearance."

Prof. Varro says as far as impeccability of temper is concerned, in Europe, my test constitutes the rule, and not the exception. Now it will be observed that none of the distinguished writers here quoted, claims that the Italian bees do not sting. They simply claim that Italian bees are more docile than common or German bees. I should be much pleased if Prof. Varro would state how he became aware of this European test of purity. I would also ask those bee keepers who have the first volume of the BEE JOURNAL, to read page 62; and I think it would conduce much towards elucidating this point, if the editor would republish that portion of Kleine's articles on Italian bees.

It further seems to me that Prof. Varro lays undue weight on the fact that queens are bought up in Italy by perambulating agents of foreign exporters, from ignorant peasants for many

miles around, to procure the required number. For my part, I cannot see any objection to this, so long as it is not shown that the bees raised by Prof. Mona are superior to those of ignorant peasants, and I must say that I could not see the slightest difference. I will concede, however, that the ignorant class of people may be incapable of raising bees that will not sting. But, so far as my recollection serves, I have not encountered any man who claimed that he had bees that would not sting, except Professor Varro. Nor would such bees suit me if I could get them. How long would it be before mischievous boys would destroy the stocks and steal the honey? In common with the Baron of Berlepsch, I would sooner submit to a dozen stings a day, than be deprived of this formidable weapon of defence.

Every experienced bee keeper knows that bees vary greatly in their behavior at different times. My pure Italian bees, and some of my hybrids, are nearly as gentle as and behave like Prof. Varro's, during the time that white clover is in bloom; but their deportment is very different at various other periods.

As to the charge of Mrs. Tupper that my apiary must be greatly bastardized on account of its rapid increase, will not Dr. S. Sanford, of Lima, Ohio, be kind enough to report what he found in my apiaries when he visited me?

And now, Mr. Editor, if you do not find this epistle too long, please insert it in the JOURNAL, as I have no doubt that many readers take an interest in the discussion. A. GRIMM.

JEFFERSON, WIS., Dec. 11, 1867.

[For the American Bee Journal.]

Profits of Bee-Keeping.

MR. EDITOR:—Seeing a statement by Mr. Baldridge in the JOURNAL for November in regard to the profits of an apiary owned by Mr. Silas Way, has prompted me to give you and the readers of the JOURNAL a statement of the products of one owned by myself and son. We had at the commencement of last winter one hundred and thirty-six stocks, very lightly stored with honey, as the season had been the most unfavorable for honey I have known during the twenty-eight years I have owned bees. Our bees came out in the spring alive, except two stocks, and both of those starved. But one large Italian left about fifteen pounds of honey which they could not get at, because there were no winter passages through the combs. After they were set out in the spring, we lost some stocks by starvation and brood rot, (foul brood) so that when the honey season opened we had about one hundred and fifteen healthy colonies. Three-fourths of these had to be fed previous to that time. We fed them on cheap sugar. Swarming commenced on the 14th of June and ended on the 14th of August; the Italians taking the lead at least two weeks and closing later by three weeks than the natives. A number of our young Italian stocks swarmed in August after filling eight boxes; and the one that came off on the 14th of August gathered honey enough to winter. We now have two hundred and

four stocks, besides one that we have sold—making ninety young stocks all in good condition. Many of our natives did not swarm at all, but I believe the Italians all swarmed.

Honey in glass caps sold.....	6,155 pounds
Strained honey sold.....	350 "
Strained honey on hand.....	250 "
Box honey on hand.....	380 "
Honey sold in family or given away	100 "

7,225 "

The above statement is not far from correct, I think, as we have kept the account.

Our bees are mostly Italian, and are as pure as can be found anywhere; bred by Mr. W. W. Cary, of Colerain, (Mass.), who possesses superior advantages for rearing pure Italian queens, and is a man perfectly reliable in all respects.

I think we have greatly increased the value of our bees by the introduction of the Italians. It seems to give new life and energy to all their movements, however slight the mixture with the natives.

I have given the amount of honey our bees have stored, and now I will give the product of a single stock of hybrids which I had in a large box hive. It cast a swarm on the 20th of June. This I put into a hive on which I use four glass boxes, and from which we took fourteen full boxes of seven pounds each. The mother stock cast a second swarm from which we took four boxes, making together eighteen boxes or one hundred and twenty-six pounds of honey. I then transferred the old stock, and should think it would weigh an hundred pounds. There being no young brood to hatch, I think the contents, apart from the hive, would weigh seventy-five pounds, which added to the surplus above-mentioned, would make two hundred and one pounds, besides three swarms in good condition to winter.

We had other Italian bees that did equally well. One cast a swarm and filled fourteen boxes, and the cast filled five boxes, besides some only partially filled, and cast a swarm. Another Italian swarm came off on the 2d of July and filled twelve boxes. Our honey was mostly gathered from clover and sold for thirty cents per pound.

DOTY BRIMMER.

HOOSICK, N. Y.

A specimen of Trebizond honey, which still retains the deleterious properties ascribed to it in Xenophon's *Anabasis*, was sent to the London Zoological Society in 1834.

There are always some people who are so forward in their belief that the very fact of seeing a statement in print is authority enough for them.—*Mudie.*

It is in those cases of which we can personally observe or perform only a part, that association and co-operation is of much value.

Send us names of bee-keepers with their post office address.

THE AMERICAN BEE JOURNAL.

WASHINGTON, JANUARY, 1868.

☞ THE AMERICAN BEE JOURNAL is now published monthly, in the City of Washington, (D. C.,) at \$2 per annum. All communications should be addressed to the Editor, at that place.

☞ We have again on hand a large number of interesting and valuable communications received too late to appear in our present issue. The occurrence of the Christmas holidays made it necessary for us to anticipate the usual time of going to press.

The Erica, or Heath-like Aster.

Dr. J. W. Hunter, of Salem, North Carolina, writes to us as follows, respecting the species of Aster recently mentioned in the BEE JOURNAL by a correspondent, as a valuable bee plant:

"I see in the last number of the JOURNAL that you speak of a plant that has been sent to you from West Virginia, and which you call the *Aster ericoides*. We have the same plant here. It is a great honey yielding plant; and if it had not been for it this fall the majority of our bees would have died before spring. You seem to doubt its honey yielding qualities, but I can assure you that it is an invaluable plant for bees. I know some stocks that gathered as much as thirty-five pounds of honey from the time the Aster bloomed until frost killed it. It was a pleasure to go out in the fields and along hedge-rows and see the bees gather honey."

In the character of a *weed*, this Aster abounds in the fields in the neighborhood of Washington. Pity it does not possess sufficient *exclusiveness* to supplant and eradicate the noxious wild garlic so prevalent in the same quarter.

☞ In response to a correspondent, we would remark that one of the primary objects of the BEE JOURNAL is to enable practical bee-keepers to communicate with each other, easily and freely, on all topics interesting to them, as such. Hives, processes and manipulations are thus expected to be brought under notice and discussion; merits and demerits pointed out; advantages and disadvantages ascertained; the valuable separated from the useless; the new and well-tested brought into favor, and the inefficient or obsolete discarded—in short, the chaff winnowed from the wheat. This, of necessity, involves inquiry, investigation and discussion, which can only be conducted to any

valuable purpose when untrammelled. To this end the BEE JOURNAL cheerfully affords room, indulging its correspondents with the largest liberty compatible with propriety; for it is a settled maxim with us that whatever will not bear discussion has no right to challenge acceptance. We much desire, however, that correspondents should treat each other courteously on all occasions.

It is, moreover, very desirable that investigation and discussion should be the work of disinterested parties—of persons sincerely anxious to aid in evolving the truth and advancing bee-culture, whether as a business pursuit, a scientific study, or an amateur hobby. We shall ever be pleased to receive and publish the observations, remarks, and comments of practical men; but would respectfully decline the favors of those who write merely to "praise up" some favorite fancy or contrivance, or to "run down" others. If from actual experience or well devised and carefully prosecuted experiment, disadvantages of any kind, in any case, have become manifest, point them out plainly that others may be benefited—regardless whether or how it affects the interests of originators or patentees. So likewise let that process receive your approval or praise which has stood the test of scrutiny, having been found valuable on actual trial. In every case, however, be sure to give us *the facts*, plainly stated, so that whatever may be thought of the accompanying comments or theories, there shall ever be something valuable added to our stock of knowledge.

In conducting this journal we have no partialities to indulge. Entertaining very decided—perhaps peculiar—notions on some points, we are not prone to obtrude them on our readers; and are quite willing that those who differ from us should enjoy their own opinions, and express them, too, when they please, in our columns.

This may seem to have a tendency to keep disputed questions unsettled. But till they are satisfactorily solved, it is better thus than that they should be summarily disposed of by the arbitrary dictum of any one. The muddiest stream will, in time, work itself clear, if allowed free course.

EDITOR BEE JOURNAL: I would wish you to tell your printer not to change again my phrasology so as to make me say just the reverse of what I wrote.

On page 116 (B. J.) where writing about what Mr. Harbison believes, after the phrase *a seminal sac*, eleventh line from top, the following sentence is left out altogether—"that she

has no control over this seminal sac, although, " &c., &c. And then, instead of beginning the next sentence by saying: And yet Mr. H. *does believe*, the negative "not" is interpolated between *does* and *believe*. Let him correct his misstatement in the next number; and please do tell him that,

"When I am writing of *freshly blown roses*, He never may make it of *freshly blown noses*."

Minor mistakes are of no consequence.

Very truly yours,

F. VARRO.

[For the American Bee Journal.]

On Wintering Bees in the Open Air.

MR. EDITOR:—Intelligent apiarians are aware that most bee-keepers in cold climates, who winter their bees in the open air, are not satisfied with their success in any style of hive yet devised. While the hives are kept tightly closed at the top, moisture condenses or freezes upon their interior surfaces, and often upon the combs themselves. In this way many colonies contract disease, or perish entirely. (See p. 340 of my work on the Hive and Honey Bee.) If the honey-boards of movable comb hives are removed and the tops of the frames covered with corn cobs, straw mats, old woolen garments, or any non-conducting, or (in common phrase) "warm" materials, it is easy to keep the bees dry without too much escape of heat.

There is, however, one source of trouble which seems to have escaped the notice of most observers. While the weather is moderately warm, the bees do not get into a very compact mass, but as soon as frosty nights come, they instinctively contract their cluster, so as to assume as compact a form as possible; even crowding into all the empty cells within the cluster. Now it would seem easy to provide in movable comb hives, all the conditions which are essential to their successful wintering, if the colonies have sufficient bees and honey. A proper number of the central combs, which have the least honey and are most suitable for early breeding, should remain in place as best adapted to the winter quarters of the bees. If necessary, some of the fuller combs should be brought nearer to the center, so that in long continued cold, the colony may not perish for want of provisions easily accessible. When no winter passages are made through the combs, the bees in the outside combs when they feel chilly, retreat from the colder surfaces of the comb to the center, and if there are not enough of them to keep up the proper temperature, they soon die. Should the weather become sufficiently warm, the bees will remove them from the hive, and on the return of another "cold snap," the same process will be repeated until by frequent losses, the hive often becomes too much depopulated to survive the winter, or too weak on the opening of spring.

The making of winter passages will not always prevent these disasters. It is true that the bees becoming sensible of the increasing cold contract their cluster, and that driven away

from the outer surfaces of the combs, they contract centrally; and it would seem almost certain that those in the outer combs, when they find themselves unable to keep warm, would, in contracting strike the winter passage, so as to get one comb nearer to the center; and if too cold there, would pass still one comb nearer to the center of the hive. The colony by contracting or expanding centrally, according to the state of the weather, would thus be always in the best condition for wintering comfortably. This theory is admirable enough, but unfortunately in practice, it often fails.

The sudden change from moderate to very cold weather, seems to deprive the bees of the admirable wisdom they so often exhibit in adapting themselves to other emergencies. They are often as foolish as human beings, who, when almost frozen, insist upon lying down to take their ease. They contract centrally to be sure; but the two outside clusters often fail to strike the winter passage, and huddling together miserably perish.

Making the walls of the hive double, or lining them with warm materials, will not prevent such disasters; but will often aggravate them by tempting the chilly bees to hug up against these comfortable substances until they are too far gone to join the central cluster, even if they knew how to reach it.

How can we best prevent such losses? I submit the question to your readers, and may, in your next number, give the results of considerable experimenting upon this important subject.

L. L. LANGSTROTH.

OXFORD, BUTLER Co., O., December, 1867.

[For the American Bee Journal.]

Chickory as a Bee Plant.

As there have been several plants recommended in the BEE JOURNAL, as furnishing pastureage for bees, I would name another—"Chickory." It is biennial or perhaps perennial, as pieces of the root will grow. The prepared root is used, especially by the Germans, as a substitute for coffee. It flowers from early in July till frost, yielding both honey and pollen. There are not many flowers on which I have seen bees work so industriously, except the raspberry. I would recommend it to Mr. Salisbury, and others, who wish to grow plants for bee feed. The seed can be obtained of R. K. Bliss & Son, 41 Park Row, and 151 Nassau street, New York, at twenty cents per package, and doubtless from other seedsmen also.

CANFIELD, OHIO.

J. WINFIELD.

[For the American Bee Journal.]

Novice wants to know how to get "Giantess" down safely from that big tree?

Very easily. Place at the root of the tree an empty hive, containing a small quantity of

's

"Bee Charm."

EXCISION.

P. S.—Use double the above quantity of the infallible "Bee Charm" if you wish to have the bees carry down the combs and honey, and deposit them nicely in the hive.

[For the American Bee Journal.]

Uniting Bees in Box or Movable Comb Hives.

I noticed an inquiry in the BEE JOURNAL, volume third, page 57, as to the best method of uniting bees. My mode of procedure is thus: If the bees are in box hives, drive out the bees you wish to unite, in an empty box. First, invert the hive containing the bees, and set the empty box on top. Then take two small sticks or use the palms of your hands, (see *Langstroth on the Hive and Honey Bee, 3d edition, page 155,*) rap the sides of the hive smartly from ten to fifteen minutes, and the bees with their queen will be found clustered at the top of the box. Now blow some smoke into the hive with which you wish to unite them, to drive the bees up among the combs. Close the entrance, and drum the hive, as in the former case, from three to five minutes, till a loud humming is heard within. Then carefully turn the hive bottom side up; take the box containing the expelled bees, and shake them into the inverted hive; set the latter right side up, and the work is done. It may be well to look at them ten or twenty minutes afterwards; and if found fighting, blow smoke into the hive for a minute or two, which will give them all one scent, and they will mingle without further trouble. But when the work is properly performed, this latter operation is seldom required.

Where movable comb hives are used, the operation is quickly performed. First blow smoke into each hive. Close the entrance, and drum them as above directed until the bees have filled themselves with honey, which will be in four or five minutes. Remove the honey-board from each hive, lift out the combs from the hive you wish to unite, and shake the bees off on the top of the frames of the other hive; close the hive, and your work is done.

HENRY S. LEE.

EVANSBURG, PA.

[For the American Bee Journal.]

Raising Queens.

I cannot make my bees raise queens from worker eggs, with any degree of certainty. Does it make any difference whether the eggs are in new or old comb?

Last spring I forced a swarm from a frame hive, giving the swarm two sheets of brood comb, and set the old hive on the stand of a strong stock, which I removed. They did not build any queen cells, though I gave them a sheet of comb containing eggs, at four different times without success. At last I gave them a sealed queen cell on Friday, and on Monday following—in three days—the queen had laid a circle of some four or five inches of worker eggs. It is now the best stock I have; but it gave me no surplus honey.

The forced swarm swarmed twice.

What was the cause of the failures?

C. T. ADAMS.

WEST MEDWAY, MASS.

[For the American Bee Journal.]

Italian Bees and Red Clover.

In reply to the question—"Has any one not raising queens for sale ever had Italian bees to work freely on red clover?"—proposed by Mr. McCune in the BEE JOURNAL, vol. 3, page 58, Mr. Long says, on page 75, "I propose to answer this question to his entire satisfaction. Mr. Langstroth requested Mr. R., myself, and several others, to step into his clover patch, which was close at hand, and satisfy ourselves on the subject. We did so, and found the Italians working upon the bloom, without seeing a single black bee."

Mr. L. may have answered Mr. McCune's question, but the answer is not entirely satisfactory to me. Are there any black bees in Mr. Langstroth's neighborhood? Mr. L. is engaged in the sale of queens. If he rears his own queens, supplying his customers with a pure article, he must of necessity have first Italianized all the bees in his immediate vicinity; in which case there would have been no black bees to work on red clover. The Italians will probably sometimes work on red clover, and so will the natives; but will they work on the red clover when the natives will not? Will they collect and store any more honey in a season, than the natives, in consequence of their superior ability to collect it from red clover? To many of the readers of the JOURNAL this question may seem of little consequence; but to beekeepers residing in localities where the main dependence for bee pasture is white clover, with but little of that, and where they are surrounded by large fields of red clover, it is of considerable importance; and some of them at least, before purchasing the long-billed variety, wish to *know the facts*. If the statements made by most of those who are engaged in the sale of queens were accepted as evidence upon this point, the proof would be conclusive. Mr. Quinby, in "Beekeeping Explained," new pages 311 and 312, says: "I had two colonies nearly all changed, several hybrids, and a number in which I had just introduced the queens. I had about sixty native colonies, and all Italians marked with the yellow stripe, which would have made about three good swarms, in one apiary. White clover was blossoming in abundance and the early red or pure clover in small quantities. Here was a chance to see if they frequented the red clover more than the natives. I found nine Italians to two natives on this plant. The two exceptions might have been black hybrids." In a note, at the bottom of page 312, he further says: "This was important to me. If the honey from white clover could sustain sixty or eighty colonies, *that from the red would sustain nearly as many more, and I could keep double the number each year.*" In his circular for 1867, he also says: "*I have no opportunity to see them work on red clover, as little is raised in this vicinity.*" What is the trouble here? Has Mr. Quinby or his neighbors suddenly ceased raising red clover? Or has he discovered that, as bee pasture, it is of no practical value to the

Italian bees? If so, "why not be frank about it, and say so?"

Mr. Langstroth says they work freely on red clover. Other dealers repeat the assertion; but most of them reside in localities where there is no red clover raised, and obtain their information from parties residing at a distance. Mr. Quinby, in his circular, further says: "The testimony that they do so is very strong. Mr. Stevenson, of Albany county, N. Y., assures me that a colony of his—the only one he had in 1864—filled the surplus honey boxes with a good quality of *clover* honey in August, while his black bees in the same yard collected only from *buckwheat*."

If disinterested Italian beekeeping readers of the BEE JOURNAL will please give, through its pages, the result of their observations and experience on this point, either for or against, they will oblige

A. BEEKEEPER.

P. S. When the above is answered, I may propose a few more questions relating to Italian bees, on points where superiority is claimed and the claim disputed—such as their being less disposed to rob; also their very amiable, kind, quiet, peaceable, mild, tractable, docile, compliant, human breath-loving disposition, &c.

A. B. K. OR VICTIM.

[For the American Bee Journal.]

The Italian Bee Question.

MR. EDITOR:—That "a little learning is a dangerous thing," was never better exemplified than on the subject of bees. A man with a dozen hives, and a few months' experience, assumes to understand everything that can happen, or ever will happen; knows the effect of a wet season, a dry one, a cold season, a warm one, a plenteous yield of honey, a season of scarcity; can tell what effect these conditions will have on wintering bees, the moth worm, foul brood, and other things, and thinks himself master of aparian science; and the less he really knows, the more he presumes. And when he has read the production of some one equally ignorant with himself, he is not content till he has thrust the obnoxious quackery into the faces of all. The result is, there can be no science whatever established. What one attempts to build up, another is interested to pull down. No one has yet a reputation for accuracy in all things, that will warrant us in repeating his experiments without watching the result. Every one that *knows* anything on this subject, *must* experiment for himself. But I protest against any further teaching of this sort. No fact should be admitted as established for one moment, without a long series of experimental observations by men of ability who can *see things as they are*, without prejudice, report without misrepresentation, and can deal *justly* with his fellow man without the compulsion of the law.

Ever since the first introduction of the Italians, there has been a raging controversy as to purity. Nothing too absurd to offer as a test, every one of which, when unsupported by some other test, can be shown to possess no reliance whatever,

If the teaching of the ignorant, dishonest and inexperienced were excluded, we might have more profitable matter. I say the ignorant, because he talks of that of which he knows nothing; the dishonest, because he slanders and misrepresents for his own or friend's benefit; the inexperienced, because he is too hasty in his conclusions to be reliable,—as has appeared in the JOURNAL for the last six months.

Does not Professor Varro destroy much confidence, in any science he may presume to teach, by his misrepresentation in the BEE JOURNAL? It gives the whole breadth and depth of his observation, and betrays the shallow foundation upon which *his* science in this matter is based. He has procured queens "from two different sources" and raised "sixteen flying ones," and sets himself up as *judge* of purity; while another, who has procured queens from near a dozen sources, and has raised thousands, he does not allow to be reliable. Also, he sets up a standard varying materially from Mr. Langstroth.

On page 19—BEE JOURNAL—he makes a garbled extract from my circular, endeavoring to show that I have no Italian bees with more than one yellow band. What was his motive? If it was *fair* and *honest*, why did he not give the whole of what I did say—at least this much: "We have no test of purity that is wholly satisfactory," and I do not remember of ever *presuming to give one*. On the contrary, I gave what I believe all would agree to be a test of *impurity*.

Prof. V. says: "To my certain knowledge the best apiarians always speak of *three yellow rings*, and so does Mr. Langstroth." As Mr. Langstroth is added as if not included in the "best apiarians," I am interested to know who they are. When a man talks about *certain knowledge*, we have a right to suppose it means something more than bombastic pretension. When he has informed us *who the best are*, I hope he will inform us *what constitutes best apiarians?* Whether aptitude for jumping at conclusions, without a single reason beyond the say-so of some one else equally ignorant? Whether morality is embraced in the word *best*—ability to report truthfully—no pretension of *knowledge* when only guessed at? Whether ability to observe correctly, waiting patiently the developments of nature before deciding? Whether the ability to manage successfully, for the third of a century, the largest apiaries in the whole country, is included?

Mr. Worthington, page 48, having a copy to improve upon, has exceeded the Professor just a little. In speaking of that article, he says: "It is certainly the fairest and most satisfactory which has been given by any American beekeeper in your paper. There must be a great deal of harm done the bee interest of the country by the immense number of impure queens sent out yearly by men who ought to know better than to go into the business before they had made themselves perfectly familiar with the markings of the Italian bee as settled by the best European apiarians." "It is surprising to see how boldly the gentleman who considers one band all-sufficient, sets forth in his circular, as tests of purity, the very marks and temper, which any one who has read volume first of the

BEE JOURNAL, or Mr. Langstroth's writings on the subject, knows are certain indications of mixed blood."

Relative to the *markings* as settled by the best European apiarists, I would ask *who* they are, and *when* it was *settled*? *Where* it was, and just how, and what it was? About going into business before becoming acquainted with all these things, it *may* be attributed to the same cause that makes D. M. Worthington assume to be umpire in this matter, evidently with borrowed experience, as is indicated by his reference to the BEE JOURNAL and Mr. Langstroth. I think I have seen a description of pure bees from Mr. Langstroth, a little at variance with Prof. V.

How to *know* "certain indications of mixed blood" by reading the BEE JOURNAL or Mr. Langstroth, is not quite clear to me. By what authority are they constituted judges? Where did they become so? I do not wish to deny them any qualification which their long experience has given; but, before I am willing to take any opinion of their's as *law*, as *knowledge*, I must understand very many points on which it is based.

We talk about the purity of breeds of horses, of cattle, dogs, fowls, and bees. How many of these pretenders can give a rational idea of what it is? How many have ever thought of the *origin* of breeds of any kind? We are told that there is a district in Italy surrounded by impassable barriers—to bees—in which this variety is found. I would enquire, how they originated? Were they created at the beginning of all things? Or grown out of the black bee by peculiar surrounding influences continued for centuries; or the result of a cross of two or more varieties—perhaps the Egyptian and black? Whatever the origin, we have proved a modification in color of queens, if not bees, in the first generation. Cross the Italians with our black bees, and we have a mixture—half the progeny black; the other, beautiful Italians. What would be more absurd than to judge of their purity by color? Take all the black ones from a colony of hybrids, and exhibit only the bright ones, and what shall prevent these color judges from being deceived? Cattle are called pure when you go back but few generations without a cross. Can any breed be *pure* but by in-and-in breeding, from the last cross? Could we take the hybrid progeny of our bees, and breed from the same strain—selecting the two opposite colors—for a few thousand generations without a mixture, would we not have a new breed distinct and fully established, and would not every branch be equally pure, though variable in color, whether brown, yellow, or even black? And under similar influences each mother might transmit an exact copy of herself for many generations. Now if there is a place where our Italian bees have been perpetuated for a period further back than history reaches, is it not certain that whoever obtains stock from that district, or direct descendants from them, will have the utmost purity, let the color be what it may? By all this, I am not endeavoring to show that *one band* on the workers is a test *all-sufficient*, but on the contrary that *no one test is sufficient—not even three distinct rings*. I claim to have as pure, as high colored,

bees as were ever imported, and when I describe them, endeavor to do it as it is. When I use the word *distinct*, I mean *distinct*. I have some bees—that are probably a cross of two importations—that when they first fly, show not only three yellow rings, but a series of rings, the whole length of the abdomen, of a color not distinctly yellow, but sufficiently near for *some persons*, who use glasses a little *green*, to describe as *all yellow*. I have others that on all ordinary occasions, show three distinct rings, but some of them, when they *first leave the cell*, when they have been stung, or the abdomen is contracted by cold, or when very old, the three rings are so nearly blended, that I cannot call each distinct in sight, although I know they are really separate. It seems to be forgotten, that to show three yellow rings distinctly, they must alternate these with, and show rings of another color, just as distinctly.

Relative to disposition, I think I should find more to agree with me when they come to have the same experience. I transfer a great many from wood to straw hives—both black and Italian—in the fall, and back again in the spring. I also ship a great many by railroad, that require extra packing, and handling of combs to secure safety, at the most irritable season. The very bees that I find quiet in the working season, are often cross now—it is the rule instead of the exception. Were my experience limited to fifteen or twenty hives in the working season only, it might be differently reported.

Did it ever occur to Prof. V. and Mr. W. that when they show clearly, that I have no pure Italians, that they make Mr. Langstroth as well as many others, rascals! I have bought queens of Mr. L. several different times, *tested queens*, paid him his full price. I received them, and to prevent any mistake afterwards, clipped one wing. He was not to send them to be tested by me—to see what they they were—but such as he already knew to be pure. He visited me, saw the progeny, at least of one, and pronounced it *beautiful*. I have described candidly, and now am I to be persuaded by such men, that Mr. L. is not to be depended upon—sending out spurious articles for genuine. It is too much like swindling, for me to believe.

I would say, in conclusion, that if Prof. V. would carefully read over a portion of the article on page 19, and imagine that I had written it, as applied to himself, he would have in substance what I would say here, should I prolong this further.

M. QUINBY.

ST. JOHNSVILLE, N. Y.

[For the American Bee Journal.]

Bees and Beekeeping in Virginia.

MR. EDITOR:—Enclosed you will find two dollars for the "AMERICAN BEE JOURNAL," with which I am much pleased; and it would have afforded me additional pleasure if I could, with this communication, forward you a long or even a short list of subscribers for the JOURNAL, as every owner of bees should subscribe for and read it.

Beekeeping, however, is at a very low ebb in

this section of Virginia. But few persons use anything better than the old box hive, without even boxes for surplus honey. They *brimstone* their surplus stocks in the fall, and get out a mixture of honey, bee bread, and young bees, not suitable to send to market or pleasant to use at home. I am the only person, to my knowledge, in this section, who is using the movable frames; and as to the Italian bees, I suppose not one beekeeper in twenty, in this part of the State, ever heard of them until I introduced them into my apiary last August. Here permit me to say that I purchased my three (3) queens from Mr. W. W. Cary, of Colerain, Mass., being advised to do so by Dr. E. Parmly, of New York—to whom my thanks are due for the advice, and for the information given me by letter, as I am much pleased with my purchase. Mr. Cary's prices are very low, as compared with those of other breeders, being ten dollars for three queens, shipped in the best manner possible—very few of the workers sent with them being dead on their arrival.

Mr. Cary, I think, must have the *genuine article*, from the markings of the queens and of the workers accompanying them, and the workers reared in the hives to which the queens were introduced—all having the three yellow bands.

A fourth ring I should consider superfluous.

Mr. E. Gallup, in his communication to the *Iowa Homestead*, which you published in the last number of the JOURNAL, gives the true theory in regard to luck in beekeeping and lucky swarms, namely—straight combs, and all or nearly all of them brood combs. I transferred such a lucky swarm to a movable frame hive this summer, and found nearly all the combs straight, and not more than six inches square of drone comb, and that in one corner where it had evidently not often been used for breeding purposes. This swarm was originally in a hive or palace, as it was called, being built with an outside casing enclosing three drawers; one below, two feet long by fourteen inches wide and twelve inches deep, as a *home*; and two above, each one foot square and fourteen inches wide, for surplus honey. It was put in this-hive in May, 1847, and never failed to make from fifty to sixty pounds of surplus honey each season, and two years made 120 pounds each. This hive seldom swarmed; but when it did, the swarms were fine and large. When transferred, I could see no difference in the size of the bees as compared with other and younger stocks, although they had been reared in combs twenty years old; and in some of the cells I could separate and count the different layers of cocoons spun by the larvae, to the number of fifteen or twenty. So much for the hobby of some venders of patent hives, for removing the combs every year, to prevent the raising of dwarfs. This hive had an entrance twenty-four inches long, which was always left open; but the colony being a strong one, defied the moth miller and all other enemies. My brother has a colony which has been in the same hive (a very large one) for thirty years, from which he gets annually from forty to fifty pounds of surplus honey.

This has been an unfavorable fall for bees in this section. Late swarms will hardly get

through the winter if left upon their stands, which is the mode practiced here. No one, to my knowledge, has tried burying, or wintering in cellars. I have, after doubling my weak stock and supplying them with honey, and some with molasses poured into their combs, removed them to my cellar. (By the way, does any of the readers of the JOURNAL know whether bees would winter entirely on molasses? A neighbor saved a colony last spring, by feeding molasses poured over crumbs of corn-bread, and set on the bottom of a common box hive.) I wish to know what the temperature of the cellar should be, and whether I have given them ventilation enough. I have removed the honey boards and the top that covers the boxes, and put on instead a cover with only two $1\frac{1}{4}$ -inch holes through it, and covered them with wire cloth, and also the entrance which is six inches long by half an inch wide. How am I to know when they have ventilation enough? The temperature of the cellar ranges from 34° to 40° . Will the bees require water during the winter, when thus housed up? An answer through the February number of the JOURNAL, or by letter, from some practical beekeeper, will be thankfully received and duly appreciated, as my valuable Italians are also in the cellar.

Will Mr. Bickford, of Seneca Falls, N. Y., let us hear from him, through the JOURNAL, as to how he is getting along with his machine for manufacturing perfect honeycomb? We shall all want the combs next season, if not the machine to make them. I have just received, from Mr. W. Dikeman, a sample of starting comb, as he terms it, being a thin sheet of wax with the shape and size of worker cells impressed on each side. I think it will be of considerable service, to attach to the frames and top of boxes, to secure straight combs; but nothing to compare with Mr. Bickford's combs, which he proposes to turn out with full depth of cells and perfect in shape.

J. R. GARDNER.

CHRISTIANSBURG, VA.

Some of the bee-raisers in California have hit upon a novel expedient to increase the product of their hives. They place the hives on a broad-bedded wagon with springs, and allow the bees to range at will on the low lands along the bay of Suisun, San Puebla or San Francisco, during the latter part of the rainy season, when the weather is pleasant, and during the early spring. As the season advances, and the flowers become more abundant on the uplands, they drive higher and higher up the mountains, the bees always returning at night to the spot where they left the hives in the morning. In time, the valleys and foot-hills become parched and bare, but the mountain heights still retain their verdant covering, and the bee proprietor ascends until the jumping-off place is reached, or the clouds in the heavens warn him of the approach of the rainy season, when he commences to descend. This system enables him to take thrice the usual amount of honey from the bees every season. It is the favorite one in Contra Costa County, around Mount Diablo.

Send us names of bee-keepers with their post office address.